

opinion respecting the corona could be made public. At present all that is generally known is, that he regards the corona as "an effect due to the passage of sunlight through our own atmosphere near the moon's place." Those are the words he used (see NATURE, vol. i., p. 14). I imagined that I had understood them. It seems I had not. Will he explain them, and perhaps indicate how the sunlight gets there? I only need to learn how one ray of sunlight can reach the atmosphere near the moon's place, during central totality in any considerable eclipse, and why the atmosphere actually at the moon's place (that is, all that cone of air which lies between the eye and the moon) is left free of this sunlight. This being satisfactorily explained, I should waive all other objections and accept the atmospheric glare theory without further question.

RICHARD A. PROCTOR

[Mr. Proctor should have quoted the context, in which Mr. Lockyer carefully refers to Dr. Frankland's and his own conclusion (or theory) "against any such extensive atmosphere as the corona had been imagined to indicate." He then states that the "conviction" that the corona is probably an atmospheric effect, "is growing stronger and stronger," if the negative evidence of the new method of observation were alone taken into account; but this is not to elaborate a theory.—ED.]

The Horse-Chestnut

REFERRING TO NATURE, No. 37, your correspondent, Eugene A. Connell, has fallen into a mare's nest in the matter of the horse-chestnut.

Country people are well aware of the impression of the horse's foot he has discovered, but the coincidence is quite accidental, and has nothing to do with the name.

"Horse" is a very common prefix, denoting largeness or coarseness, in the same way that the prefix "dog" indicates inferiority and contempt. Thus we have horse-chestnut, horse-bean, horse-radish, horse-mint, horse-parsley, horse-leech, dog-rose, dog-violet, dog-berry (the berry of *Solanum nigrum*), &c.

These prefixes are common to nearly all languages; we have *ἵππο-κρημνος*, a horse-laugh, "fièvre de cheval," a violent fever, and a host of like terms.

Bath, July 27

CHARLES EKin

The "English Cyclopædia"

If the Editor of the "English Cyclopædia," in his letter contained in your issue of July 7, had restricted himself to defending his own handiwork, and had abstained from denying the correctness of my statements, I should not have ventured to ask for space in your columns to reply to him.

In opposition to my statement that I looked in vain for "*Arvicole*, *Crocidura*, *Crossopi*, *Hypudæi*, *Sorices*, shrews, and voles," the Editor asserts that "all the species mentioned in the Close Time Report are described or noticed in the Cyclopædia." This may be and probably is quite true. I merely asserted that they were not to be found under their respective names. I have stated that I found *Hypudæus* and the voles under the heading *Murida*. In return for my information he now tells me that if I wished to become acquainted with *Crocidura* and *Crossopus* I ought to have turned to the article *Sorocida*. But how is an unlearned reader like myself to know where to turn? The Editor only confirms the accuracy of my statement as to the great want of cross references. If the Index and the Supplement had contained such references as *Hypudæus* [Murida, E.C.], *Crossopus* [Sorocida, E.C.], &c., I should probably never have given public utterance to my troubles. In reply to my assertion that I looked in vain for an article on *Rhizocrinus*, I am told, much to my astonishment, that the proper place to find it is under *London Clay*. In my ignorance I had sought for it under its own name, *Apicrinites*, and *Encrinites*. According to this mysterious system of arrangement, if I had complained that there was no article on *Sparrows*, I should probably have been told that I ought to have looked for a notice of them under the heading *London*.

In my letter I gave a list of twenty-three important subjects on which there were no articles. In defence of these real or apparent omissions the Editor, after making the strange assertion that two of these, *Acclimatisation* and *Deep Sea Dredging*, belong rather to the "Arts and Sciences" than to the "Natural History" division, goes on to say that most of the subjects stated by me to have been omitted "do occur." He

must be well aware that I never asserted that they "do not occur." I simply said that there were no special articles on them. He might have had the candour to notice that I unearthed from their hidden recesses the subjects to which he expressly refers in his letter, viz., *Eophyton*, *Eozoon*, *Hyalonema*, and *Protoplasm*.

As I must not trespass further on your space, I will conclude by observing that I fully concur with the Editor in the opinion that "what a Cyclopædia ought or ought not to contain is an open question;" but when an Editor has the moral courage to assert, in illustration of the mode in which he discharged his functions, that "*Meloe* was inserted and *Sphexidæ* rejected, because there was not room for both," and gives no less than twelve columns to the former instead of dividing the space between the two; and when he tells us that *London Clay* is the proper place to seek for information regarding *Rhizocrinus*, the readers of NATURE may draw their own inferences as to what a Cyclopædia, under his superintendence, is likely to be.

I must add that I have not the slightest idea who the Editor of the Supplement is, and that until his letter appeared, I did not believe in his existence.

South Devon, July 8

NEMO

Entomological Inquiries, etc.

I WAS much interested, two nights ago, at finding on the wall of my drawing-room a flattish, dark-grey winged insect, six or seven tenths of an inch in length, which, on being placed in the hand, exhibited two small but brilliant sparks of light towards the extremity of the tail. In the imperfect light in which it was examined, the wings seemed to have elytra and the body to be somewhat like a small caterpillar, with a tapering tail. In size and general aspect it resembled the Italian fire-fly, with which I made acquaintance last summer on the Lake of Como, without, however, a sufficient examination to justify more than the most superficial comparison. My knowledge of entomology is so defective, that I feel unable to form an opinion whether it might be that insect or the male of the common glow-worm (which, however, is not common in my neighbourhood). If so meagre a description may enable any of your readers to give me satisfactory information as to this point, I shall feel much obliged to them.

There is adequate evidence that some kind of fire-fly has been seen during hot seasons in England. Kirby and Spence give a reference, which I have no opportunity of verifying, to Phil. Trans. 1684, as to their appearance in Hertfordshire, and their having been considered the genuine *Lampyrus italica*. The following unpublished account may be interesting as having come to me from a perfectly reliable source: "In 1822—the year is pretty certain—during the month of June or July, the weather being very hot, on at least two evenings a number of fire-flies were seen at a village near the Thames, between Reading and Henley; they were flying about the fields and the lawn before a gentleman's house, and some of them came into the house; three or four or more might be seen at a time, like little flying lamps. The insect was brown [reference is then made to a sketch from memory thirty-two years afterwards, from which it must have greatly resembled my specimen], and seems to have had opaque elytra and network wings; the light was in the tail, like that of a glowworm, as bright, but probably not as large. A very intelligent gentleman who was upon the spot, an acquaintance of Dr. Wollaston's, who had been in America and the West Indies, was greatly astonished; he caught some of them, and considered them identical with the West Indian firefly. He said he had heard of their being in England, but never seen them."

A lady, whose experience must be referred to a later date than the foregoing account, has informed me that she once observed them for a single day in Wiltshire.

The newspapers of 1868 or 1869—I am not certain which—spoke of them as abundant in some places; particularly, I think, at Caversham in Kent, where they were even considered "nuisances!" if I recollect right. Some of the readers of NATURE may perhaps be able to furnish information as to this alleged fact.

There is something very remarkable in the occasional appearance of these beautiful insects in our climate. They can hardly be thought to reach us by direct migration. Can it be supposed—as it has been ingeniously suggested to me—that their ova are frequently being imported from warmer countries, but are only fully developed in the temperature of our hottest summers?

While speaking of entomological matters, allow me to mention that in the month of August last year the small blue lobelias in my garden were the favourite resort not only of my hive-bees, but of a species of wild bee so singularly resembling them in every respect (excepting, perhaps, a barely perceptible amount of greater firmness and roundness of form), that they could only be distinguished by the presence of a tuft of lemon-yellow hairs in the front of the head between the eyes. I thought at first that this might have resulted from a lodgment of pollen, but it soon became evident that it was a specific distinction. These pretty insects were very numerous, but I never found any nest. Will some apian reader oblige me with an identification?

To turn to a somewhat different subject. In a little book called "Flowers of the Year," published by the Religious Tract Society, is the following passage: "An interesting phenomenon is sometimes exhibited by red and orange-coloured flowers, and also, in a less degree, by yellow-tinted blossoms. It is that of a light of their own colour playing about the plant. This is not the result of an inflammable vapour igniting on the approach of a candle, but seems rather, as Sharon Turner has remarked, 'an actual secretion of light additional to their usual show.' The cause of this phenomenon has not been discovered, but it seems dependent on an electrical state of the atmosphere. It has not been seen during the bright sunshine, but has been observed after sunset in several flowers, as the marigold, the different species of poppy, the scarlet geranium, and even in the heartsease." I have several times met with a similar statement, and much wish to know what trustworthy foundation there may be for it. I have repeatedly tried to verify it by observation, but in vain.

Since penning the above remarks, the yellow-visored bee has again appeared on the blossoms of a *Linaria Alpina*, the descendant of a plant brought by my wife from Switzerland seven years ago, and now blooming profusely with us. I have seen several specimens of my old friend, but cannot as yet satisfy myself that the yellow tint is due to hair of that colour, as I supposed last year.

Hardwick Vicarage, July 12

T. W. WEBB

The Solar Spots

I HAVE been much interested of late in observing the solar spots, and especially in the greatly-increased numbers which have lately made their appearance. I have counted from 100 to 200 spots, through a six-foot telescope, with a power of 100, quite frequently in the past few months. On the 22nd ult., with a power of 200, I counted 675 sun spots, and on the 27th saw 470 with the 100 eyepiece. In general, I think the number of spots visible is about in proportion to the power used when the atmosphere is favourable for high powers. During the month several spots were visible to the naked eye.

I feel quite desirous to learn what our spectroscopists find about the sun's margin, also if observations indicate a terrestrial magnetic force corresponding with the sun's activity.

Would not many readers of NATURE be much interested with results from Huggins and Lockyer and the Kew observers?

Spiceland, Indiana, July 6

W. DAWSON

DARWIN BEFORE THE FRENCH ACADEMY

THE discussion on the claims of Mr. Darwin for election into the Zoological Section of the Paris Academy was continued at the meeting on August 1 in *comité secret*, and the *Revue des Cours Scientifiques* gives a report, of which the following is an abstract, of M. de Quatrefages' brilliant and able reply to M. Blanchard:—There are two men included in Mr. Darwin, a naturalist observer and a theoretical thinker: the naturalist is exact, sagacious, and patient; the thinker is original and penetrating, often just, sometimes too rash. That the theory with which his name is connected, that of Natural Selection, has in it at least something seductive and plausible, is shown by its having been worked out by such men as Darwin, Wallace, and Naudin, labouring independently and in different paths. If the ideas and the works of Darwin are such as some of his opponents represent, how can they have obtained the support in less than ten years of such men as Lyell, Hooker, Huxley, Karl Vogt, Lubbock, Haeckel, Filippi, and Brandt himself, who has just been

elected correspondent in opposition to Mr. Darwin? In Darwin's great work there are certainly some things which are found in Lamarck, the laws of heredity, and the transmission and progressive development of characters. The point of departure of Lamarck is an incessant spontaneous generation, that of Darwin is a unique archetype which he supposes to pre-exist, and the origin of which he does not seek. That which belongs to Darwin alone is the laws of variation which he has established, and the law of correlation of growth. His error has been the confusion between the laws which regulate the foundation and propagation of races and of species; substitute the former for the latter and his theory is incontrovertible. Without defending Mr. Darwin's theories, some of which he has indeed publicly combated, M. de Quatrefages then proceeded to enumerate the various branches of scientific inquiry in which Mr. Darwin has made original observations, and concerning which he has contributed works of great importance to science. In geology we find seven great memoirs—1. On coral islands; 2. Geological observations on volcanic islands; 3. Geological observations in South America; 4. On the connection of the volcanic phenomena in South America; 5. On the distribution of erratic blocks in South America; 6. On the geology of the Falkland Islands; 7. Origin of the saliferous deposits of Patagonia. In botany the speaker invoked the testimony of Dr. Hooker that the most beautiful discoveries made during the last ten years in vegetable physiology belong to Mr. Darwin. Finally, in zoological literature we have the report of the voyage of the *Beagle*; and the monograph of the Cirrhipedes, one of the most important monographs ever published. After speaking of his more popular works on the origin of species and the variation of animals and plants under domestication, M. de Quatrefages referred to his important and laborious investigations of the strange variations in fowls, pigeons, and rabbits; and summed up his eloquent address as follows:—"En résumé, M. Darwin est un naturaliste éminent qui veut écarter de la science l'invocation de la cause première, et chercher l'explication des faits naturels du monde organisé dans les causes secondes, comme on le fait depuis longtemps en géologie, en chimie, en physique. Mais il ne va pas au delà, et il ne faudrait pas juger Darwin sur la parole de quelques disciples qui semblent parfois ne pas avoir lu ses ouvrages. Il y aurait injustice à le rendre responsable des exagérations et des aberrations de ceux qui s'abritent sous son nom."

M. de Quatrefages was followed by M. Ad. Brongniart, who attacked the Darwinian system, denying the existence of variation in plants. The appearance of species is a fact which can only be explained by a supernatural cause, and Darwinism is nothing but a fairy tale. M. Ch. Robin considered that in respect of proved facts which he had introduced into science, there would be a hundred zoologists who should have precedence over Darwin. M. H. Milne-Edwards replied to M. Brongniart, that the sea sometimes discloses fairy tales, and spoke of the very great value of the monograph of the Cirrhipedes. Although himself opposed to Darwinism, he strongly supported his nomination. M. de Quatrefages, in reply, denied the charge against Darwin made by M. Blanchard, that he had declared that man was descended from the apes. In deciding Mr. Darwin's claims, we ought not to be influenced by those points in which we have to combat his views, any more than Lamarck was judged in this manner. In spite of his errors, he will be none the less one of the glories of science and of the Academy. His nomination will not make the Academy Darwinian. Men of science know that the Institute appreciates work independently of doctrine, and men of the world know that the supporters of Darwin in the zoological section, MM. Milne-Edwards, and de Quatrefages, have always professed themselves opposed to his ideas. The discussion was then adjourned to last Monday evening.